

of English inventive genius. The over-shadowing influence of the recent sensational inventions of the telephone and phonograph have led even practical men to believe that inventive power had crossed the Atlantic, but no one who reads Mr. Culley's book can fail to learn how much has been done in England. Though duplex working was revived by Hearn, and quadruplex made practical by Edison, neither was invented in America. On the other hand, Hughes's beautiful type-printer was born in America, but it was developed in Europe, and its birthplace knows it not. Thomson's syphon recorder, Varley's double-current translator and condenser working, Bain and Wheatstone's automatic systems, fast-speed translators, and all the valuable systems and apparatus in use for testing have sprung from here, and are well described in this work. The Post Office telegraph system, in its technical department, is a credit to this country and a pattern to the world, and it possesses on its staff some of the most practical electricians of the day. Messrs. Preece, Lumsden, Marson, Gavey, and Kempe are well known everywhere, and though their labours are not acknowledged by Mr. Culley, it is well known that they have contributed materially to establishing the telegraphic system of the Post Office. It is especially in developing the automatic system and in establishing fast-speed translators that the Post Office officials have been so successful. A relay station in Anglesey has increased the rate of working [between London and Dublin from 70 to 120 words per minute. Translating relays working at the rate of 120 words per minute are quite new in telegraphy. Mr. Culley has given scant justice to Mr. John Fuller for his new form of bichromate battery, a battery that is coming into very extensive employment for all purposes. It is a zinc-carbon couple, the exciting fluid being Poggendorff's mixture. Its peculiarity consists in the shape of the zinc, which is permanently inserted in a bath of mercury. Its electromotive force is double that of a Daniell's cell, its constancy wonderful, its economy great, and its cleanliness and freedom from smell all that can be desired.

This work is deservedly popular, not from its literary merit, but from the position of the author and from the great mass of very valuable practical information it possesses.

OUR BOOK SHELF

Manual of the Vertebrates of the Northern United States, Including the District East of the Mississippi River and North of North Carolina and Tennessee, Exclusive of Marine Species. By Prof. D. S. Jordan, M.D. Second edition, Revised and Enlarged. (Chicago: M^cClurg, 1878.)

THE object of this volume is to give collectors and students a ready means of identifying the families, genera, and species of the vertebrate animals of North America. Following the usage of botanists, the author has adopted the system of artificial keys to the classes, orders, families, genera, and species, while use has been freely made of every available source of information. The account of the mammals has been chiefly compiled from Prof. Baird's work, and Dr. Coues has given great assistance in the part relating to the birds; while in this edition the account of the fishes has been entirely re-

written in order to include the results of recent investigations in that department. The fact that a work of this nature should in two years' time call for a second edition, is, indeed, a proof of the interest taken in natural science by the American people. This edition seems to fairly represent the present state of knowledge.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Phonograph and Vowel Theories

SEVERAL letters have appeared in NATURE bearing on the subject of the phonograph, and referring to our first communications upon the subject. We are glad to see that our statement as to the reversibility of consonants (NATURE, vol. xvii. p. 423) is generally accepted. We feel that as yet the phonograph does not speak with sufficient clearness to determine how perfect this reversibility is, and that the effect of many minute parts of articulate utterance cannot be heard with any certainty. Mr. Ellis, in his first communication, ranked the phonograph somewhat too low, but we are more than satisfied with the acknowledgment in his second letter (vol. xviii. p. 38). Mr. A. M. Mayer and Prof. Sylvanus Thompson both speak of the marks on the tinfoil as differing according to the distance of the mouth from the diaphragm. We do not observe any effect of this kind and see no theoretical reason for any alteration in the relative phases of the simple tones with a change of distance from the mouth. Mr. Mayer seems here to have fallen into an error. We find ample confirmation of Helmholtz's statement that the phase relation between two constituents is not appreciated by the ear. Each person usually, but not invariably, adheres to the same phase relation on one pitch, but different people pronouncing the same vowel with approximately the same constituents, combine these differently, which, as Mr. Mayer says, would make reading the marks on the tinfoil a very difficult matter.

With reference to the letter by Mr. C. R. Cross which appears in NATURE, vol. xviii. p. 93, we adhere with much confidence to the opinion that the five vowels, *a e i o u* (Italian), pronounced in succession, are by contrast at least thoroughly distinguishable when the instrument is run at various speeds, such as to reproduce the sounds at all the pitches within the compass of the average human voice. That no marked change is produced in the relative values of the vowels is confirmed by the fact that neither in public nor private exhibitions do the hearers of sentences alternately run slow and fast suggest that the vowels have changed with a change of speed. This alone would be a sufficient proof that *oh* does not change into *ae*, as we understand Mr. Cross to say, and there is no ground, according to Helmholtz's theory, for expecting that it would. To us the relative sounds of the vowels at various speeds seem at least as perfect as those obtained from Willis's well-known experiment, where a succession of vowels is suggested by contrast when the length of a resonating tube is altered.

We do not, however, think that our instrument speaks with sufficient distinctness to warrant our expressing an opinion as to the constancy of quality of any single vowel when the instrument is run at various speeds.

Some *ohs* remain apparently very constant, and at times we thought that other *ohs* became brighter or more like "awe."

Sometimes we thought *awe* became very like "ah." We should be glad to learn the impressions of any of your readers as to this point.

We venture, however, to remind any one trying the experiment that a low note followed by a high one suggests a change from *u* (Italian) to *i*. Thus if we whistle a low note and then the octave to it or a note near this, the ear is easily persuaded that the whistle resembles *ui*, but if now, beginning again on the note we just thought was *i*, we go up another octave, the new sequence again suggests *ui*, although the very note which was last taken to represent *i* now stands for *u*. If, therefore, we wish to judge what a sound really is we should not trust much to contrast, especially when a change of pitch is involved in the comparison.

We have now obtained and analysed a very large number of vowel curves, especially for the sounds *o* and *u*, and with your permission will send a selection of these for publication after the results of our investigation have been communicated to the Royal Society here. These curves show what the voice effects when singing at different pitches a vowel which remains of constant quality according to the appreciation of the speaker. The analysis of the curves gives the partial tones of which the vowel sounds are composed, and it becomes a matter of considerable interest to see how far these results confirm or contradict existing theories.

We therefore propose to give a short sketch of these theories, hoping that if in error, we may be at once corrected.

Prof. Willis showed (Cambridge *Phil. Soc. Trans.*, vol. iii.) that, by varying the length of a tube attached to a free reed, he was able to produce the sensation of a change of vowel sound. This sensation is not very definite, especially for the vowels *i* and *e*; but, when the length of the tube is changed rapidly, the ear accepts the suggestion of a change from *u* to *o*, *a*, *e*, and *i*. Prof. Willis concluded that the vowel quality was given in each case by the coexistence of the note proper to the tube with that of the reed, whether the former was or was not harmonic to the latter. One must bear in mind that Willis wrote before it was recognised that all musical sounds are compounded of harmonic partial tones, and also before the function of a resonator was understood. This we may call the absolute single-tone theory. Wheatstone pointed out that the tube used by Willis acted simply as a resonator.

F. C. Donders (*Donders' Archiv*, vol. i.) observed that when the mouth was formed to speak a given vowel, the cavity had a certain definite pitch of resonance, or maximum resonance, which he determined by observing the pitch of the whispered vowel. Donders and Helmholtz agree in considering that this characteristic pitch is nearly constant in man, woman, and child, for a given vowel. Donders also observed that the vowels were, in certain cases, accompanied by a "geruisch" or "whish."

Helmholtz attacked the subject in a different way. By means of resonators he applied a qualitative analysis to the sounds which came out of the mouth cavity when a vowel was spoken, and pointed out that a vowel-sound at a particular pitch was characterised, not by a single tone, but by many tones. In an early paper, translated in the *Phil. Mag.* for 1860, he describes not only the analysis by resonators, but the synthesis by means of tuning-forks, which is now a familiar experiment. In this paper he appears inclined to believe that it is the relation between the constituent tones which determines the vowel quality; that, for instance, any pair of simple tones one octave apart will, if properly proportioned, always make an *o*. This theory made considerable way; it is taught with small qualification as to absolute pitch in Tyndall's lectures on sound, and elsewhere; it may be called the relative-pitch theory.

Helmholtz himself does not seem ever to have formulated it, for although the paper referred to distinctly suggests it, he guards himself by saying that the relations observed can only be considered as proved for the particular forks used, and, in fact, his experiments were only made at two parts of the scale.

In the "Tonempfindungen" the relative-pitch theory is entirely abandoned, but it is not a little difficult to ascertain what Prof. Helmholtz's latest theory is. This difficulty is indeed admitted by his translator, Mr. A. J. Ellis.

By Herr von Quanten, whose papers were published in *Pogendorff's Annalen* for 1875, Prof. Helmholtz was understood to mean that for each vowel one, and in some cases two, tones of definite absolute pitch must be strongly present, these tones being those which Prof. Helmholtz calls the characteristic tones of the vowel. This would imply either that each vowel could only be sung on a very few notes, or that the characteristic tones were present as inharmonic partials. Neither of these conclusions being in accordance with fact, von Quanten concluded that Helmholtz was wrong; but Mr. Ellis, with justice, as we think, points out that the true conclusion ought to have been that Helmholtz could not possibly have meant to broach an absurd theory.

We confess that we were ourselves led to believe at first that Helmholtz taught that each vowel contained strongly either its characteristic tone or some of the higher partials of that tone or tones very near these, and that this was what gave it its distinctive character. This was the theory which our first experiments seemed definitely to contradict. We now believe, however, that this is not the doctrine taught by Helmholtz.

Indeed we fail to find in the "Tonempfindungen" any very complete vowel theory, but we think that the following passage, taken from Donders' pamphlet "De Physiologie der Spraakklanken, 1870," expresses very clearly a doctrine which is very generally looked upon as that of Helmholtz.

Donders says (if our translation from the Dutch be correct):—"Vowels spoken loud are sounds of a determinate timbre maintained unaltered, depending on the form of the mouth-cavity and of the mouth-aperture, and, even without the accompanying 'whish,' characterised by strong comparatively low upper tones not occurring in a definite order relatively to the prime tone, but for each vowel of an approximately constant pitch."

We understand Donders to believe that on whatever part of the scale a vowel be spoken the pitch or pitches of maximum resonance of the cavity are constant for a given vowel, and that indeed the form itself is constant. This may be called the *constant cavity theory*, and is taught by Mr. Ellis as the doctrine of Helmholtz.

We fail to find that Helmholtz himself has stated this doctrine definitely in all its rigidity, although he accepts the results of Donders' experiments, and has himself confirmed and amplified them. Almost every statement made by him concerning vowels is limited to those which he could produce by forks forming an harmonic series with B₁ for the prime. Any experiment described by Helmholtz is of course to be relied on, and so far as we have yet traversed the ground we find that the phonograph gives results in accordance with his experiments as to these constituents, but when we examine the one or two more general statements made by Helmholtz we find room for doubt, both as to his meaning and as to the truth or completeness of the conclusion.

Thus at the end of Chapter V. Helmholtz says:—"Vowel qualities of tone consequently are essentially distinguished from the tones of most other musical instruments by the fact that the loudness of their partial tones does not depend *only* on the numerical order but *chiefly* upon the absolute pitch of those partials; thus when I sing the vowel A to the note E₁ the reinforced tone B₁ is the twelfth partial tone of the compound; and when I sing the same vowel A to the note B₁ the reinforced tone is still B₁, but is now the second partial." The two words marked by us in *italics* have been introduced for the first time in the fourth edition.

This passage might very well be understood to mean that a certain tone of perfectly or at least very approximately definite absolute pitch must necessarily be present in a given vowel. Further examination has, however, convinced us that Prof. Helmholtz does not require the presence of any characteristic tone or of any one of a group of characteristic tones in a vowel. This is made obvious by Chapter VI. In this chapter, which treats of artificial vowels, we find that in order to make an *e* by tuning-forks Prof. Helmholtz employed B₁ and B₂ as "being adjacent to the deeper characteristic tone f₁," which in fact lies midway between them; in the same way he employs f₁, a₁B₁, and B₁B₂ for the same vowel, treating all these as adjacent to the higher characteristic tone B₁B₂. Thus the theory of Prof. Helmholtz is satisfied if tones lying anywhere within a whole octave be present, provided the characteristic tone lie somewhere near the middle of that octave. This is consistent with Donders' statement of the theory, provided "approximately constant pitch" be allowed to signify anything within six semitones.

We consider the following abstract as representing the doctrine taught in the "Tonempfindungen":—

1. For a given vowel there is a certain form of mouth cavity which has a pitch (sometimes two pitches) of strongest resonance—as B₁ for *o*.
2. If this vowel be spoken or sung on any subtone of this pitch, the overtone corresponding to that pitch will be strongly present.
3. If the same vowel be pronounced at some other pitch then these harmonic partials will be reinforced which lie within, say, six semitones of the characteristic pitch.

No opinion seems to be expressed on the following two points:—

1. Whether the mouth cavity for a given vowel remains constant when the pitch of the vowel is altered. Mr. Ellis understands Helmholtz to affirm this, which is apparently Donders' view, but we have failed to perceive any passage in which this is definitely asserted. Helmholtz says the cavity for a given vowel has a pitch of strongest resonance, but this is not

quite the same statement as saying that when that vowel is spoken at all pitches the same cavity is employed.

2. Whether the mouth-cavities for given vowels are supposed to differ phonetically *only* in respect of pitch of maximum resonance. Helmholtz states clearly that in respect of their pitch of maximum resonance they are different, but he does not clearly say whether or no any other differences are essential. There are passages which seem to show that he considers that any resonator of the required pitch (whether in the least like the mouth in shape or material) would answer as well, or nearly as well, as the special mouth-cavity for the production of a given vowel. On the other hand it is at least conceivable that the cavity for, say, *o* may be very different from that for *a* in other respects than simply in the pitch of maximum resonance. As to this we find no statement in the "Tonempfindungen."

In fine we do not see that Prof. Helmholtz, although he has largely added to our knowledge concerning vowels, has laid down any law by which, given the pitch at which any one vowel is to be spoken, the reinforcement of its constituent tones could be even roughly predicted. This prediction could, however, be roughly made upon the constant-cavity theory, and has been made by Mr. Ellis in his valuable additions to the translation of Helmholtz's work. Prof. Helmholtz seems to do little more than tell us the constituents of a series of vowels sung or said on two notes of one scale, coupled with one peculiarity and in some cases two peculiarities of the resonance cavity. He has avoided all general conclusions except that quoted above, which states that the vowel peculiarity depends chiefly on the absolute, and not on the relative pitch of the partials.

In our next communication we hope to be able to state how far the information we have derived by means of the phonograph contradicts, supports, or supplements the above theories.

Edinburgh, May 29

FLEEMING JENKIN
J. A. EWING

Extinct and Recent Irish Mammals

I BEG to thank Prof. Leith Adams for his criticism, in NATURE, vol. xviii., p. 141, of my "Preliminary Treatise on the Relation of the Pleistocene Animals to those now living in Europe" (*Palaen. Soc.*, 1878), in which, from the nature of the work, it is impossible that mistakes should not be. I cannot, however, plead guilty to some of the mistakes which are placed to my credit:—1. That "the Irish elk is placed among the pre-historic mammals in consequence of its presence in the peat-bogs of England, Scotland, and Ireland." What I wrote (p. 6) was that the presence of the extinct Irish elk in the peat-bogs, which are of well-ascertained pre-historic age, renders it impossible to accept Sir Charles Lyell's definition of the term recent, in which no extinct species are stated to occur.

Of course the Irish elk, as Prof. Leith Adams remarks, has long been known to be met with, almost universally, in the lacustrine marls underlying the peat, and it is thus described in p. 27 of Mr. Sanford's and my own Introduction (*Palaen. Soc.*, 1866). I do not know of its occurrence anywhere *in peat*, but at the bottom of peat-bogs, to which the bones of animals suffocated in the peat in all probability gravitate. It seems to me very unlikely that all the remains at the bottom of peat-bogs belong to a period before the peat was accumulated.

2. I have never held, and still less to my knowledge printed, that "man and Irish elk, reindeer, mammoth, horse, and bear, were contemporaneous in Ireland." Evidence of palæolithic man, the contemporary of the mammoth in Ireland, is, so far as I know, altogether wanting. If Prof. Leith Adams will kindly write me a reference to any such statement of mine it shall be corrected at once.

My list of Irish animals, which merely purports to give the principal historic mammalia, does not profess to give all the mammalia, which will doubtless be fully treated in Prof. Leith Adams' promised work.

W. BOYD DAWKINS

Owens College, Manchester, June 9

Alternate Vision

MR. GALTON'S remark (NATURE, vol. xviii. p. 98), that "sometimes the image seen by the left eye prevails over that seen by the right, and *vice versa*," leads me to describe a curious defect in my own eyesight, which in a different way confirms what he says. While my right eye is fairly long-sighted, my left eye is very short-sighted. For instance, the focal distance

of my right eye for your leader type is 18 inches, and for the left eye only $8\frac{1}{2}$ inches. For your letter type the focal distance for the one is 16 inches, and for the other $6\frac{1}{2}$ inches. This is by the light of a Duplex lamp, and by focal distance, I mean the distance at which I can see distinctly. The result of this inequality in my two eyes is that the right—or long-sighted one—involuntarily closes when I read, and I am not aware of its being shut, except when some one who is a stranger to the peculiarity calls attention to it. During the day, however, in looking about both eyes are generally open, though when I look intently at a distant view, I find the short-sighted eye shuts occasionally. But in a general way both eyes are open, and I have two distinct images presented to my brain, one blurred and indistinct, even for faces a yard distant, and the other clearly defined, I believe, to the usual distances. How is it that my brain or mind rejects the blurred image and chooses the distinct one, so that I see everything perfectly clearly. If I get a piece of dust in the good eye, or close it, I immediately see the blurred image, and if this take place in the street, it causes a painful degree of confusion as to distances, &c., so that I am often brought to a standstill by such an occurrence. That both images really are presented to the brain I know. For instance, in travelling by train I frequently amuse myself by placing my eyes so that the short-sighted eye sees a portion of a scene through the window, without the good eye being able to see it. Then I see the blurred image only; but as the train moves the blurred is replaced by the bright one, as the good eye gets to work. The blurred image always appears at a higher level than the other, and it is the same when I shut my good eye for a moment and look at the fire with my bad one. On reopening the good one the blurred fire appears slightly above the bright one, and the latter almost instantly drives the indistinct image away—like a dissolving view. Things appear, as a rule, much flatter to me than to people who enjoy binocular vision. I know this because I have a pair of spectacles so arranged as to equalise my sights. When I put them on, objects like trees put on a delightful fullness and roundness to which I am usually quite a stranger, and the effect is most charming. I may add that two of my brothers have a similar defect of vision.

May 31

J. I. R.

The Eskimo at Paris

I HAVE read with great interest in vol. xviii. p. 16 of your renowned journal the article concerning the Eskimo, the exhibition of whom in Paris, &c., has recently made so great a sensation.

Unfortunately, it seems to me, the writer of the article, M. A. Bordier, has been incorrectly informed with regard to the introduction of these people. It is not to Mr. Geoffroy St. Hilaire, the director of the Paris Jardin d'Acclimatation, but to M. Charles Hagenbeck, the well-known and intelligent dealer in wild animals of our town, to whom science is indebted for the introduction both of the Eskimo, the Hamran and other types of the different tribes of Nubia, and the Laplanders.

I should be much obliged to you if you would kindly insert the above correction in an early number of your journal.

Hamburg, May 28

J. D. E. SCHMELTZ

The Telephone

HAVING seen a paragraph in NATURE communicated by Mr. Severn, of Newcastle, New South Wales, describing a method of using a telephone to enable deaf persons to hear, I have tried the experiment in the manner Mr. Severn describes—by fastening a string to the parchment diaphragm of a simple telephone made of wood, and carrying this string round the forehead of the deaf person, who clasps the string with both hands and presses them over his ears. The experiment in this way was partially successful; the sound of the voice was always heard, and some words were distinguished. Afterwards I fastened a single string to the telephone and got the deaf person to hold the string between his teeth. He then heard every word distinctly, even when spoken in a low tone of voice at the whole length of the room.

63, Strand, W.C.

JOHN BROWNING

TILL now I have looked in vain for any account in NATURE of experiments with the telephone or phonscope, inserted in the circuit of a selenium (galvanic) element (see NATURE, vol. xvii. p. 312).

One is inclined to think that by exposing the selenium to light,